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16
OBSERVATIONS

ON THE

RECENT OFFICIAL REPORT

ON

FEVER AND DYSENTERY.

BY

JOHN MACPHERSON, M. D.,

FIRST ASSISTANT SURGEON, GENERAL HOSPITAL.

“ Was there any novel mode of practice introduced,—was the nature of that practice beneficial, and such as to render its general adoption desirable ?”

Medical Board in 1816.

Calcutta:

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F. CARBERY, BENGAL MILITARY ORPHAN PRESS.

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TO C. MOREHEAD, Esq., M. D.,

Bombay Medical Service.

&c. &c. &c.

MY DEAR SIR,

I TAKE the liberty of addressing the following remarks to you, as one of the most constant and successful cultivators of practical medicine in this country.

I wish that they were of a less controversial nature, but, however undesirable in our profession controversy may be, there are occasions on which it is impossible to avoid it.

On the first appearance of the Bengal Report on Fever and Dysentery, I published a statement, which showed how far its conclusions were borne out by the records of the General Hospital, and having then examined the Tabular Statements of the Report that referred to the Institution with which I was connected, I thought that I had done all that was necessary on my part. But though the Analysis was, I believe, satisfactory to those who were in some degree

conversant with the facts of the case, and though its statements remain at the end of seven months unchallenged, it has, I have been told, been generally considered very much too concise, and the following pages are meant to remedy that defect.

Although also I knew that there was a very general wish throughout the profession in Bengal, that a more extended reply should be given to the Report, I remained silent, hoping that some influential member of the Service would take up the subject. But as I now see no prospect of this, and as besides, I have access to information on this particular subject, which no one at a distance can possess, I again venture to come forward.

•

The following observations, in which the Report is treated, purely as a matter of medical science, and with as much freedom, as if it had not been issued in an official form, fall under three heads:—

- 1st.—An examination of the nature of the experiment within the wards of the General Hospital.
- 2nd.—A review of the Tabular Statements.
- 3rd.—An inquiry into certain points of Theory and Practice.

In the last part I shall not follow the Experimenter in all his speculations, but confine myself chiefly to some points on which absolute originality has been claimed, and of which the success, according to the official account, "has been triumphantly demonstrated."

I cannot pretend to absolute accuracy in all the numerical statements which I have made, but I believe that their general correctness may be depended on. For such inaccuracies as may be discovered (and I shall feel obliged to any one who will point them out to me) some apology will, I hope, be found in the great difficulty which there is, in dealing accurately with the crowded general tabular Statement appended to the Report.

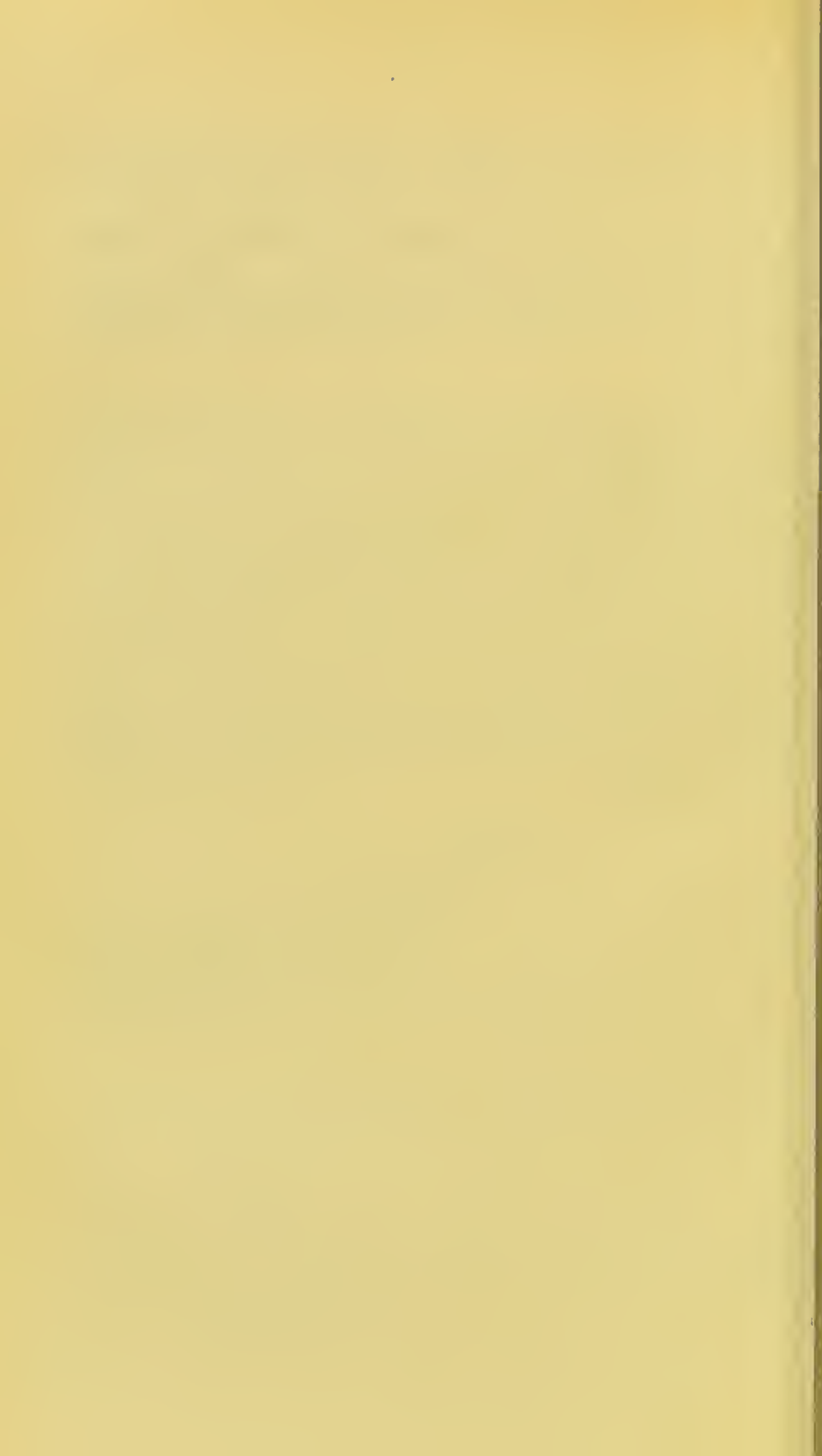
I am,

My dear Sir,

Yours very truly,

JOHN MACPHERSON.

November 8th 1852.



I. ON THE NATURE OF THE EXPERIMENT IN THE WARDS OF THE GENERAL HOSPITAL.

“ Equidem dicam ex animo, Patres conscripti, quod sentio, et quod vobis audientibus saepe jam dixi.”—CICERO IN PISONEM.

THE Medical Board, in an early part of their Report remark, “that the method of figures, extracted from a wide field of observation, is the least fallible of all methods.”

True it may be in the abstract, that there is only one method that can give solid results in medicine, that which is based on numbers, and supported by exact observation and accurate induction. But then the numbers themselves must be arrived at with the utmost care and consideration: the figures must represent facts, which have been tested and indisputably proved. If the numbers are hastily assumed, the air of method that belongs to numerical calculations, is only an additional source of error and confusion. Yet even when the numbers are correct, and all the bearings of the cases they represent, have been well ascertained, they may not afford a basis wide enough for any general conclusion.

Hence it is not surprising, that many practical men look on this method as a very poor test for the comparative value of different modes of treatment, and that it has even been seriously said, that it is possible to prove anything regarding a given mode of treatment by means of numbers—the homœo-

pathlists, for instance, referring to an array of figures as evidence of their success.

The numerical method has, in short, failed often, and in such instances of failure, mainly from its imperfect application.

There are two conditions for obtaining valuable results from any experiment in medical science—the experiment must be conducted with extreme care, and it must be made on a considerable scale. Both these propositions may be almost termed self-evident, but the first of them deserves more minute examination. I shall now therefore inquire, what points should be borne in mind in an investigation of this kind, and how far such points were attended to in the experiment at the General Hospital.

1st. It is of primary importance to ascertain and lay down definitely, what the particular mode of treatment is to be—for if it be a mixed one, it is extremely difficult for the most discriminating practitioner to ascertain what effects are really produced by the individual remedies which he employs.

What was the treatment in the Experimental Ward? Was it a pure and unmixed one? Solely Quinine and Arsenic, the long tube and copious injections? This I imagine is the common opinion. Yet general bleeding and leeching, purgatives, opiates, metallic astringents, &c. &c. were all used in that ward. Has it been ascertained to what extent they were used? What degree of influence over disease was to be attributed to them, and what to the first-mentioned remedies? Was it ascertained in bowel complaint how much was effected by quinine, how much by the other remedies, and how much by injections? Was it even ascertained how often on an average the long tube was passed, in each case? On all these points information is required, and until it is fur-

nished, the experiment must remain a crude and imperfect one.

2ndly. To make the results practically useful, it should be accurately ascertained for what forms and for what stages of disease, a given mode of treatment is peculiarly suited. But in the published results of this experiment we have no Tabular Statements, showing in what varieties of fever and of bowel complaint the treatment was most successful. All the cases of Fever appear under one head, and the cases of Diarrhœa and of Dysentery are also for the Experimental Ward not distinguished from each other, much less acute and chronic dysentery, or the complication of liver affection. As to treatment—"Plenty of quinine" is recommended for all fevers, and for malarious dysentery; it is apparently not recommended for acute dysentery, but then we are not told how to distinguish the two. In short the tendency of the report, and of the practice recommended in it, is, mainly owing to want of precision in defining the experiment, and stating its results, to encourage careless observation, and want of attention in making out the changing phases of disease and their periods, considerations very important as regards its safe and successful management. The serious mistakes that may arise from the want of precision alluded to in this and in the preceding paragraph, are well illustrated, by contrasting in another part of these observations the results of the quinine and non-quinine treatment in the Experimental Ward.

3rdly. For comparative purposes, it is necessary that the patients treated should be all generally of the same class, and that the admissions should take place continuously to all the wards, which are the subject of comparison, as we all know the effects of season in modifying disease. Now, so far as I am able to follow the Tabular Statements of the

Report, the cases of women and children (the latter a class usually yielding a high mortality) a description of patients not treated at all in the Experimental Ward, are included in the Returns with which those of the Experimental Ward are contrasted—and besides this, the admissions to the different wards were interrupted. At one time the Experimental Ward took in all the cases of fever and of bowel complaint, then again it ceased for a few days to take any in, while at another time it only took its share of them with the other wards, and at still another, it admitted a few cases of neither fever nor bowel complaint. All these are disturbing elements, and must detract from the value and accuracy of the results.

The absence of children would tell in favor of the Experimental Ward, the other irregularities might or might not be in its favor according to circumstances. Regarding the influence of chance in these matters some remarks were made elsewhere, and every day affords fresh illustrations of it.*

4thly. It is very necessary to ascertain the exact condition of a patient on leaving Hospital. Boudin, when conducting his experiment with arsenic before a committee, kept his fever patients in hospital for 15 days after they were cured, that there might be a certainty of no relapse. At the General Hospital no such condition was observed. No one saw the patients before they were discharged, except the Medical Officer who had treated them. The Hospital Returns give no list of partial recoveries: they have only the headings "Discharged" or "Died," for the headings "Transferred" and "Invalided" give little information. The discharged are all supposed to be cured. Yet how many of the discharged may have died soon after in other hospitals, and how many

* "Analysis," p. 11.

of them were re-admitted*? This remark of course applies equally to all the wards of the General Hospital.

5thly. A patient, if not rejected at once as an unsuitable one for the experiment, should be treated from beginning to end, in the ward into which he has been admitted. He is not to be set down as a case of fever or of bowel complaint, if he be not one of either, but he ought to be written off to the total general results of the ward into which he was admitted. Now at the General Hospital, (where I may explain that there were three wards, an Eastern, Western and Experimental, each under a separate Medical Officer,) this system was only partially adopted. Out of the 24 deaths occurring in the Experimental Ward, 16 are written off, as not being cases of fever or bowel complaint, and they consequently do not appear anywhere in the Report, while at the same time two dozen cases, one half of which proved fatal, were, after an average of four days' treatment, sent, as not being cases of fever or bowel complaint, to the other wards,† instead of being written off like the rest. This has necessarily led to inextricable confusion of cases; it has, however, afforded an opportunity of judging of the effects of the treatment in the Experimental Ward in diseases, other than fever and bowel complaint.

6thly. A committee, or at least one person besides the experimenter, should watch the progress of the cases from day to day, and be present at the *post mortems*. No such precaution as this, was, as far as I know, adopted, and the duty would no doubt have been an irksome one. Still to obtain satisfactory results some such measure was absolutely

* See *infra* Dysentery. A reference to the records of the Medical College and Scaman's Hospital is desirable.

† If this had been the uniform practice, it would have been, comparatively speaking, unobjectionable.

necessary, and has always been adopted in inquiries of a similar nature.*

Regarding the other condition which has been mentioned, the sufficiency of the scale on which an experiment is conducted, every one will admit generally that, in drawing inferences from facts, the fewer the number of them, the less valuable will be the conclusions to which they lead, and every one will also, I presume, admit that in the present instance the scale of the experiment was small. It seems therefore hardly necessary to enlarge on this topic.

And thus, the want of method in conducting the experiment, the short period of time over which it extended, and the smallness of the number of patients that were the subject of it, would appear to deprive the experiment of almost all value.

As, however, in the opinion of the Board, one season at the General Hospital has produced clear and positive results, it may be well, although enough has been said to show that the basis on which the experiment itself rests, is not a very secure one—to examine the Tabular Statements adduced in proof of its success.

* Many of these points were, during the course of the experiment, brought to the notice of the Medical Board by the experimenter or by myself.

II. TABULAR STATEMENTS.

“ Behaupten ist nicht beweisen.”

I. THE TABULAR STATEMENTS GENERALLY.

1. The most general expressions of the Medical Board on the subject of the success of the Experimental Ward are the following:—“We have prepared tables to prove the absolute reality of the superiority, positive as well as comparative, which he claims for the methods of treatment pursued by him over those pursued by others,” and the experimenter remarks “I have actually had only one-half and one-fifth the mortality of my neighbours,” *i. e.*, the Eastern and Western Wards of the General Hospital and of H. M.’s 70th.

2. The position of the Medical Board is supported by the only abstract which they make, that of the results yielded by the general comparative statement; and it is a comparison of the percentage of mortality of the two Experimental Wards, one in the General Hospital and one in that of H. M.’s 70th, added together, with that of the other wards of the General Hospital and of H. M.’s 70th, and of all other European troops serving in Bengal, added together. It is as follows:—

	<i>Diarrhæa.</i>	<i>Dysentery.</i>	<i>Fever.</i>
Experimental wards, ...	0	4·91	0·7
All others,	3·24	10·55	1·24

3. If the Board had only this general comparison of results before them, their strong commendation of the Experimental Ward is not surprising, as it is apparently fully borne out by figures. But it is just possible that in their anxiety “to pre-

vent cavilling at the narrowness of the field of observation" they may have run into the opposite extreme, and have selected too " wide a field of observation." It seems improbable that they would have arrived at the foregoing conclusions, if they had first closely examined the facts occurring within the narrower field of observation, and then proceeded to the wider one, using the facts supplied by the latter chiefly as illustrations. This is the course which I shall endeavour to pursue.

The special examination of the returns will presently show that these results are based on inaccurate data. But apart from this, some general objections must be made here to the nature of this comparison.

4. It classes and compares the results of the treatment in the various wards of the General Hospital, with the general results of the army. But every one knows that the class of patients that frequents a General Hospital, is very different from that which fills Regimental Hospitals, and that therefore the two cannot be considered fair subjects of comparison. In like manner, the character of disease varies so much in different places in the same season, that the making the comparison too wide a one, may defeat the very object of the comparison. Thus to take by no means an extreme case, from the tabular statement, H. M.'s 3rd Dragoons, at Umbala, lost only 2 per cent. of its cases of bowel complaints, while H. M.'s 98th at Peshawur, lost 9 per cent. at the same time. Could this difference be attributed solely to difference of practice?

5. Another general objection to the returns is, that, for the Experimental Ward they appear to give the cases of dysentery and of diarrhœa, added together under the head of dysentery, as will be immediately shewn, but that in the other returns diarrhœa and dysentery are very properly classed separately. On this account I shall throughout

compare the returns under the head of bowel complaints generally—and this I do, not from choice, but from necessity, as under these circumstances it is the only mode, in which the comparison can be a just one. Though this is forced on me, I regret it less, for reasons which will be constantly recurring in these remarks, and here I must enter on what is somewhat of a digression, but is a matter of considerable practical importance, in ascertaining the mortality from bowel complaints.

6. There can be no doubt in the abstract that for scientific purposes and for defining modes of treatment, diarrhœa and dysentery should be treated separately, as also acute and chronic dysentery. But on a former occasion, when compiling some tables of the mortality from dysentery, I remarked* “ of course the mortality in returns may be made to vary, according as cases are classed under the heads of diarrhœa or of dysentery, a point often requiring nice discrimination.” I even found extreme difficulty in separating hepatitis from dysentery, owing to their frequent association.

The following are some of the reasons why diarrhœa and dysentery cannot well be classed separately with much accuracy in general returns. (*a.*) One man (I know this practically) will often regard as a case of chronic dysentery, what another looks on as diarrhœa. (*b.*) Many cases first admitted into hospital under the head of diarrhœa, are, when their symptoms become more marked, transferred to the head of dysentery, and the fact may very easily be lost sight of. (*c.*) On looking over a series of years, as for instance in the tables of the General Hospital and of the Garrison, appended to the Report, it appears, that though the mortality from diarrhœa or dysentery taken singly may vary much, yet the results for bowel com-

* Bengal Dysentery, p. 4.

plaint in general are wonderfully uniform; thus, in my opinion, showing the eases to be returned as dysentery or diarrhœa, very much according to the taste of the Medical Officer.

(d.) On looking over the general tabular statement attached to the Report, we find that much mortality in dysentery is frequently attended with small mortality in diarrhœa, and *vice versa*, a fact which probably also depends mainly on the same cause, as the uniformity of mortality in bowel complaints just alluded to; for instance:

In H. M.'s 70th	the mortality for diarrhœa was	2·2	for dysentery	42.
„ H. M.'s 60th,	„ „	1·3	„	18.
And <i>vice versa</i> , H. M.'s 98th,	„ „	8·5	„	9·2
A detachment of Recruits,	„ „	8.	„	7.

(e.) Another reason is, that on reviewing the results among the European troops on a large scale during the last 20 years, as given in the table at the end of the Report, it appears, that the number of eases of dysentery has been rather diminishing, while that of cases of diarrhœa, has increased, facts not easily explained by natural causes, but of which the possible explanation may be, that cases of bowel complaint are not set down as eases of dysentery, so much as a matter of course now, as they were formerly.

On the whole, therefore, I trust, that the reader will be inclined to agree with me in considering it useful, to correct any extreme results got for dysentery alone, by a constant reference to the mean average for bowel complaints.

7. As far as I am able to ascertain, the cases of women and children, (the latter a class always yielding a very high mortality), a description of patients not treated at all in the Experimental Ward, are included in the returns, with which those of the Experimental Ward are contrasted. The absence of children from any ward would of course reduce the mortality in it.

Having stated the preceeding objections to them as they now stand, I proceed to the special examination of the tables, and I shall first turn to those for the General Hospital.

II. TABLES OF GENERAL HOSPITAL.

1. THE tabular statement assigns the following comparative percentage of mortality to the Experimental, and to the united Eastern and Western Wards of the General Hospital.

	<i>Diarrhœa.</i>	<i>Dysentery.</i>	<i>Fever.</i>
Experimental,	0	7·3	0·79
Eastern and Western, ...	2·1	15.	1·4

But, on the returns from which these deductions are made, two preliminary observations must be made, one of paramount, and one of secondary importance.

2. According to the tables, not a single case of diarrhœa occurred in the ward devoted to fever and bowel complaints, during a period in which forty-seven cases of that disease occurred in the other wards.* This is at first sight a startling fact, but fortunately it can be easily explained, as the experimenter has in his own handwriting fully and fairly recorded in the public registers of the hospital, about thirty-five cases of diarrhœa and about sixty-six of dysentery, from which the ninety-eight cases of dysentery in the tables must have been obtained. It is obvious, therefore, that these ninety-eight cases cannot be regarded as so many cases of dysentery, but of bowel complaint generally, *i e.* diarrhœa and dysentery taken together.

* If I may venture on a conjecture, those forty-seven cases appear to me to represent fifteen cases of diarrhœa that occurred in the other wards, and thirty-two that occurred in the Experimental, added together by some strange confusion. The tabular statement gives the other wards eighty-two cases of dysentery, whereas I can find only thirty-eight in the hospital records,—and curiously enough, the addition of the forty-seven cases of diarrhœa to the thirty-eight of dysentery gives a close approximation to the number eighty-two.

3. The tables would also seem to imply that the Experimental Ward did not treat any military patients in the General Hospital, whereas there was no difference between the wards in this respect, of which I am aware; it will therefore simplify matters, if we first add together for the other wards the two separate columns headed military and non-military, and then contrast the results with those of the Experimental Ward, as has been done in the above abstract.

4. But taking the tables as they stand, the Board's emphatic commendation of the treatment in the Experimental Ward is hardly borne out, and the statement of the experimenter that "his mortality was only half that of his neighbours" is most certainly not supported by them, for the general tables assign (taking the totals in fever and bowel complaint) only a superiority of 1.52 to the Experimental Ward, this being the difference between 3.64, the percentage of mortality of the Experimental, and 5.16, that of the other wards. Even if the experimenter's statement be limited to bowel complaints (for with these tables the separate consideration of dysentery is out of the question), it is still equally inaccurate, for the tables assign the Experimental Ward a mortality of 7.3 and the others one of 10.2 per cent.

Now, under the circumstances of the case as already detailed, and especially the absence of women and children from the Experimental Ward, the superiority of the treatment in that ward appears to me, even according to the tables, to be wonderfully small.

5. But having discovered the mistake about the diarrhoea cases, and finding it, after many fruitless attempts, impossible to reconcile the figures in the hospital books with those in the tables, (for the tables say there were 180 admissions of dysentery, and 338 of fever, while the books say they were 88, and 268, respectively,) I scrutinized those records,

and from data furnished by them shewed (Analysis, Calcutta, March 1852) that not challenging the number of admissions assigned to the Experimental Ward, (as I can see how they are got, though I cannot see how the numbers are got for the other wards) the mortality in fever and bowel complaints was in the Experimental Ward 4.46 per cent., and in the other wards taken together 6.14. To this I am now able to add, that this somewhat smaller mortality in the Experimental Ward was caused solely by its not treating women and children, for if those classes of patients be struck off from the other wards,* their mortality was only 4.2 per cent.: consequently their mortality was absolutely somewhat less, than that of the Experimental Ward.

6. It was also shown that for fever and bowel complaints, the mortality in the Western Ward of 2.9, was considerably less than the 4.46 of the Experimental, being rather less both in fever and in dysentery.

7. Further, that the total mortality of cases of all diseases in the Experimental Ward, even after thirteen fatal cases were removed from it to the other wards, was 10 per cent., or much the usual average of the hospital for all diseases: and that there also was in the Experimental Ward a special and unusually high rate of mortality, not falling short of 80 per cent., from certain diseases, (such as apoplexy, epilepsy, ebrietas, scorbutus, pneumonia, hæmorrhoids, &c.) not being either fever or bowel complaint.

8. To this I can now add, that the total mortality from December 8th 1849 to October 31st 1850, of all cases first treated in the Experimental Ward, (and therefore including the fatal transfer cases) was 13.8, and the total mortality from all diseases treated throughout in the other wards (and therefore excluding the transfer cases) was 8.5, giving the large

* The mortality of children in the other wards from fever and bowel complaints was 13 per cent.

balance of 5.3 in favor of the other wards. As the other wards had to treat along with the common and less fatal maladies, the bulk of the most fatal diseases, such as cholera, hepatitis, delirium tremens, and also of an epidemic of small-pox, besides women and children, and as in the Experimental Ward considerably more than half the cases were of fever and diarrhœa, neither of which classes of disease yield a high mortality, the excess of mortality on the part of the Experimental Ward is remarkable. Even crediting the other wards with the fatal transferred cases, the balance is not turned, for they were still somewhat more successful, the numbers being then, 9.4 and 10 respectively.

This last comparison from its nature does not admit of being a very accurate one, but still it is by no means unimportant in its general bearings on the question.

9. I cannot consider, as some have done, that the last conclusions are foreign to the matter in hand, especially in the absence of accurate data for individual diseases. On the contrary, I regard them as of much importance in judging of the general results of the treatment in the Experimental Ward, for where is the practical gain, if a seeming diminution in the mortality from fever and bowel complaints is attended with an increased mortality in other diseases? Unless the fact of the two occurring simultaneously can be shewn to be a matter of accidental occurrence, it must materially diminish the general value of the treatment in the Experimental Ward.

In another point of view also one of the last conclusions is of much importance, for assuming the tables to be correct, it has been shewn that eighteen cases of diseases other than fever and bowel complaints, were attended with sixteen deaths.* Now, this is so highly improbable, that it seems to point to some radical fallacy in the numerical statements, or in the classification of diseases.

* Analysis, p. 5.

10. I have also thought it worth while to enquire whether the presence of the experimenter affected in any degree the total mortality of the General Hospital. As his number of cases was small, no very striking effect could well be produced; still his great success in fever and in bowel complaints ought in some degree, however trifling, to have reduced the total mortality of the season for all diseases. The total mortality for the last five years has been.

In 1847-8	9.2
48-9	13.4
49-50	11.3
50-51	10.8
51-52	9.4*

* It may be interesting to contrast these percentages of mortality with those of some European General Hospitals. The most important points to ascertain, and without a knowledge of which such comparisons are little more than matters of curiosity, are, the class of patients admitted, and the nature and the stage of their diseases, also the prevalence of local epidemics; thus the mortality in the Edinburgh Infirmary during an epidemic of typhus is at least doubled. The following data refer chiefly to periods before 1840, and are compiled from Quetelet, Porter, and Milne Edwards :

Paris—Hotel Dieu,	...	14.
Charitè,	14.	
des Enfants,	22.5	
Pitiè,	12.2	
Lyons—Hotel Dieu,	...	9.
Montpellier, all Hospitals, ...	10.	
Pesth,	16.6	
Berlin,	16.6	
Vienna,	16.6	
Dresden,	14.2	
Munich,	11.1	
Brussels,	11.1	
Amsterdam,	12.4	
Milan,	16.6	

England.	
Bartholomew's,	7.62
London Hospital,	11.44
St. George's,	11.19
Dreadnought, (1849)	11.
Manchester Infirmary,	7.16
Liverpool ditto,	5.57

Probably the class of patients in the Dreadnought comes, of all the foregoing, nearest to that in the General Hospital; but in it no doubt there is a comparative preponderance of pulmonary, in the latter, of abdominal, diseases. The average mortality in the General Hospital, when taken

for large periods, seems to have remained nearly uniform since the commencement of this century. There has certainly been no diminution of mortality. It is curious to observe that the presence of cholera since 1817, does not seem

Now the experimenter was present during portions of the seasons 49-50, 50-51, and the considerable diminution of mortality in the first of these seasons as compared with the one immediately preceding it, and a further slight diminution in the second, would look as if his presence had reduced the mortality; but on looking back, we find that in the year 47-48 the mortality was less than in either of the two years during which he was present, and on looking forward, we find that after his departure the mortality was also smaller. Any first impression of his having diminished the mortality, is therefore at once dissipated.

11. I am quite aware that there are various ways of explaining these facts, and that as they at present stand, if we were to attempt to draw any positive conclusions from them, they would merely furnish an additional example of the fallacy of the numerical test, where every element has not been taken into consideration.

One conclusion, however, to which they point, ought not to be overlooked. In the years 49-50, 50-51, especially in the latter, I find that in the annual returns of the General Hospital there was a considerable diminution in the mortality from bowel complaints, although the general rate of mortality for all diseases was not affected. This bears out the result, which we had already arrived at by another process, that apparent diminution of mortality in certain diseases must have been accompanied by increased mortality in others, and it is not a conclusion very flattering to medical science.

to affect the general averages, nor could we guess the presence of a severe epidemic of small-pox from the returns of 1849-50. The General Hospital does not admit natives, and it has few Surgical cases; the mean number of patients treated annually during the years mentioned in the text, was 1336.

III. RETURNS OF H. M.'s 70TH.

1. According to the general table, the percentage of deaths in the Experimental and other wards of H. M.'s 70th was, as under :

	<i>Diarrhœa.</i>	<i>Dysentery.</i>	<i>Fever.</i>
Experimental	0	4.09	7
Others	2.2	42.	1.4

For the examination of these returns, I do not possess the advantages which I have with regard to those of the General Hospital. I have no means of examining the books of H. M.'s 70th, or of any other regiment, nor any means of scrutinising their returns. My examination of them must, therefore, be a comparatively meagre one. Yet, a very cursory glance at the general tabular statement, excites the suspicion that there must be some fundamental error in them, as in those of the General Hospital.

2. As I understand it, the medical charge of certain companies was given to the experimenter, while the remaining and main portion of the regiment was treated by its own surgeon. Now the tables state, that the Experimental Ward had only 1 case of diarrhœa, whereas during the same period the surgeon had 135 cases of that disease. They also assign to the Experimental Ward 248 cases of dysentery and 1 of diarrhœa. The fact, of 1 case of diarrhœa occurring in the returns of the Experimental Ward of H. M.'s 70th, is much more surprising to me, than that none should have occurred in that ward of the General Hospital. Because it looks as if the cases of diarrhœa and of dysentery had been carefully examined, and separated intentionally, and produced the singular result of 1 case of diarrhœa to 248 cases of dysentery in the Experimental Ward of H. M.'s 70th ! a fact if possible more surprising, than that all the diarrhœa cases but one in the 70th, should have fallen to the surgeon. Then

again, the surgeon is set down as having 18 deaths out of 42 cases of dysentery, or the unheard-of mortality of 42 per cent., a mortality that must have depended on some more general cause, than any mere difference in the mode of treatment. These facts alone are sufficient to convince any practical man, that the returns require revision, even setting aside the fact of minor importance, but a surprising one, which also appears from these tables, that the main body of the regiment did not furnish so many cases of bowel complaint or of fever, as the companies treated by the experimenter.

3. But even taking the tables, as they stand uncorrected, I do not see how they bear out the assertion of the experimenter, if he means it to be a general one, that he had only one-fifth the mortality of the rest of the regiment, for the mortality in fever and bowel complaints, taking the totals, is set down as 2.8 in the Experimental and 5.6 in the other wards. He therefore had one-half, not one-fifth of their mortality, or even limiting the remark to bowel complaints, he still had about one-third instead of one-fifth the mortality. In either case, he is shown to have been very inaccurate.

It is further evident that in making this statement, he cannot have regarded all his own cases, as cases of dysentery, for in that event he was entitled, according to the tables which give him a mortality of 4 per cent. and the surgeon 42 per cent., to have made a boast of having only one-tenth the mortality of the other portion of the regiment.

It ought to be easy to clear up these strange-looking statements of the Report, by a reference to the regimental books, as I suppose in the Regimental Hospital there was no confusion about transfer cases.

IV. OTHER REGIMENTAL RETURNS.

1. To turn to the regiments generally, for which the abstract of the large table given by the Board is sufficiently accurate, and which has already been given (p. 7), there is the one great error, which runs through all the statements, the setting down diarrhœa and dysentery, separately for the other returns, but in reality classing them together for the experimental ones, for no one can maintain, that a ward open for the treatment of bowel complaints could by any possibility have its cases all dysentery, and no diarrhœa. If the diarrhœa and dysentery cases be added together for the other returns, and then the general result for the whole tables be compared with the Experimental Ward, the superior success of that ward is at once reduced to 1.6 per cent. according to the tables as they stand, and without applying to them any correction.

2. In fever also, the simple addition to the returns of the Experimental Ward, of one overlooked fatal case of fever in the General Hospital,* at once reduces its apparent superiority to ^{.22}~~22~~ per cent.

3. But after all, if we examine them in detail, do the uncorrected tables "prove the absolute reality of the superiority of the treatment in the Experimental Ward?" Suppose that we rigidly apply the numerical test, and ascertain whether, in bowel complaints for instance, (as the Board considers the results in them most striking,) it was the most successful. The experimental treatment in H. M.'s 70th gives, according to the uncorrected tables, a mortality of 4.08 per cent.

* Analysis, p. 8.

The Artillery, ..	Benares	had 49 cases, no death.
Ditto,	Dum-Dum	66 cases, 1 death.
H. M.'s 3rd Drag.	Umballa	lost 2.04 per cent.
96th,	Cawnpore	„ 4.3
Artillery,	Lahore	„ 4.3

Possibly this list might be extended, but this is enough to shew that by the tables themselves, the mortality was less in some regiments, and scarcely greater in other corps, than in the Experimental Ward of H. M.'s 70th. In these cases, was the success attributable to improved modes of treatment? and would it be incorrect to apply to them the remark of the Board concerning the Experimental Ward "that they have indisputably proved that their method of treatment is absolutely superior to that commonly practised?"

4. But it may be said, that, although some regiments in other stations were more successful than the Experimental Ward, still its mortality in bowel complaints must have been much less than the ordinary one among troops similarly situated. Nevertheless, if we turn to the table at the end of the Report, of the mortality among regiments stationed in Fort William during the last twenty years, and therefore, except so far as difference of season is concerned, situated exactly as H. M.'s 70th, we find that in five of these years, 1837, 39, 41, 42, 44, the mortality among them from bowel complaints was less than the 4.08 of the Experimental Ward, and in the years 1830 and 1836 about the same. Nay that even looking on the cases of the Experimental Ward as cases of dysentery, in 1836, 41 and 42, the mortality from dysentery of troops was just the same. Therefore, as compared with the average of years, no one can maintain that there was any particular success in the Experimental Ward of H. M.'s 70th.

5. But, to make the comparison a more general one (and it is in accordance with the principle of the tabular state-

ment, although not in my opinion a correct one), the tables assign to the Experimental Ward (those of the General Hospital and H. M.'s 70th taken together) a general mortality of 4.9, which is at once increased by the addition of one overlooked case in the General Hospital* to 5.21.

The 2nd Europeans lost 5 per cent.

H. M.'s 87th	5.2
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H. M.'s 22nd	5.2
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H. M.'s 75th	5.3
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and therefore were one of them more successful than, and the other two or three as successful as the Experimental Ward.

6 The question may be viewed in still another light. The treatment in H. M.'s 10th and 80th regiments is praised in the Report, as approaching in some measure to that of the Experimental Ward. We should, therefore, applying the numerical test, expect to find the returns for those regiments very favourable, yet on examination we find, that H. M.'s 10th having lost 7 per cent., is behind all the regiments above enumerated, and H. M.'s 80th having lost 8 per cent., is behind them, and also behind H. M.'s 32nd and 24th regiments. This is comparing the results of those regiments for bowel complaints generally, but if the comparison were limited to dysentery, the general results would not, I believe, differ materially.

7. The preceding remarks have been chiefly confined to bowel complaints, but the same numerical analysis might have been applied in a more striking way to fever. Thus a hasty glance shews that at least one dozen of the regiments mentioned in the table were more successful in fever, than the Experimental Ward of H. M.'s 70th, and also that the mortality of troops in Fort William from fever has, on more than one occasion, been less. But a minute enquiry into

* Analysis, p. 8.

these matters would fatigue the patience of most readers, and therefore I shall not enter on it, notwithstanding that the Report says : "The tables shew inecontrovertibly that the experimental treatment of fever was more successful than the practice followed generally in European Hospitals in Bengal," a remark still more applicable to the regiments that were more successful than the Experimental Ward.

8. What conclusions then are we warranted in drawing from the preceding analysis? I trust that every one has been convinced, that there must be many very serious inaccuracies in the returns of both the General Hospital and of H. M.'s 70th, and, as respects the "triumphant demonstration," that, whether as compared with the other wards of the General Hospital, or with the Army generally, the Experimental Ward did not present any particular or singular success, even according to the tables, compiled "in the Medical Board Office, to prove the absolute reality of its superiority."

III. ON SOME POINTS OF THEORY AND PRACTICE.

Non obtusa adeo gestamus pectora Panni :

Nec tam aversus equos Syriâ Sol jungit ab urbe—ÆNEID, Lib I.

THE assiduity and perseverance which the experimenter has displayed, are worthy of all commendation ; but bearing in mind the history of the doctrines and treatment advocated by him, the praise of originality cannot be accorded to him ; and I wonder that he should now have repeated his song of triumph,* and that the Board, though expressly declining to enter upon the question of originality, should, to a certain extent, have recognised it, by the use of the phrase, “demonstrated the perfect safety and wonderful efficacy of quinine on the heroic scale,” an expression scarcely applicable to anything that is not new. Let us now examine some of his statements respecting fever.

FEVER.

(a.) *Cause.*—One of the objects which the experimenter says in his final report he came to Calcutta to accomplish, was

* Report *passim*.—“It has been my glory to be the first to do this.” “In this I am original, and hold my own ground.” “This is my own, all my own, for who has ventured on it before ?” “*My system* contains truths of infinite importance to the health and safety of nearly the whole human race.” “I have come to open new and wide paths of discovery.” “I have done this as it seems to me almost by a *special Providence*.” Had the experiment been less successful, according to his ideas, would he have still appealed to a special Providence ? Such appeals are often very infelicitous, not to say profane. A Surgeon in a local school, addressing his pupils once said—“You thus see, Gentlemen, what Providence, backed by a skilful Surgeon, can effect.” !!

to "re-classify tropical disease," and in an earlier report he announces that, "it will be his endeavour to shew what fevers have malaria for their origin, and what depend on other causes." But of these objects having been attained, there is no indication in the Report. It was the more necessary for him to have set these matters finally at rest, as, according to his views, there would then be no difficulty in knowing when to give quinine, of giving which injudiciously, he remarks, "this accident, (namely, of great mortality in some cases of small-pox and measles,) did occur in my ward." But the discriminating remarks of the Board on this point, make it unnecessary for me to enlarge on the subject.

(b.) *Inflammatory nature*.—1. Treating of the complications of fever, the experimenter remarks—"How astonished will Mr. Martin be to find that all these congestions are not inflammations." Is this a fair representation of the views of the only man, who after leaving these shores has attained distinguished professional eminence at home? Mr. Martin writes—"almost all our complications in the fevers of Bengal are abdominal, whether they be of an inflammatory nature, congestive, or of mere irritation."—*Tropical Diseases*, p. 124. So far was he from expressing an opinion that all these complications were inflammations. The views of my friend Dr. Mackinnon are also strangely distorted, when he is made to assert absolutely "the inflammatory nature of fever"—whereas he expressly says in his very practical work, p. 234, "the other abdominal complications of remittent fever, are rather those of congestion, than of inflammation."

2. The experimenter has ascertained, by examining the records of the General Hospital, that the fatal cases of fever were "cases of extreme congestion, without a trace of the only mark which can be relied on of inflammatory action, *viz.*, the effusion of plastic lymph." But he can scarcely, I

should suppose, have come to Calcutta, expecting to find deposits of plastic lymph. I imagine that none of us are so far behind the progress of medical science, as to regard the lesions of fever as simply those of acute inflammation. But supposing that these were discoveries at the General Hospital, that this was new to us in India, and that deaths from fever, with no *post mortem* appearances at all sufficient to account for death, were of unusual occurrence,* supposing that these truths had never before flashed on the mind of any one in India, but the experimenter, is he aware that he is merely repeating the established doctrines of Europe? The late Dr. R. Williams, a very intelligent writer, says in his *Elementary Principles of Medicine*:—"As a general rule in all tropical fevers, the traces of diseased structure are always trifling," and in this he merely agrees with the French, who of late years have become far more familiar with remittents than the English, from the intimate connexion of France with Northern Africa. Grisolle, (who says expressly, that quinine is the only specific for remittent fever,) in his popular manual, which had reached a 4th edition in 1850, treating of the management of what the French call "pernicious intermittent fevers," says—"We are fully persuaded that the acute pains and the extreme derangement of the organic functions which characterise them, are not allied to inflammation," and of the pathology of remittent, he writes—"Some have ascribed the origin of remittent fever to visceral irritation or inflammation, but the researches of pathological anatomy do not bear out this opinion—for the lesions of the stomach are not sufficient to account for the general symptoms, and no one would attribute the functional disturbance of the system to any alteration of the liver, for its condition

* I gave the results of some such *post mortems* in the *Medical Gazette*, for 1841.

most certainly is no more of inflammatory origin, than is engorgement of the spleen. In fine, the researches of Leonard and Folley, in 1845, on the composition of the blood in fever, exclude the idea of inflammatory action."

(c.) *Use of Quinine.*—But physicians in India and in all parts of the world have long shown, by their treatment of fever, their practical disbelief of the inflammatory nature of these congestions. They have been gradually abandoning general blood-letting, and using quinine more freely. To vary my authority, and quote a popular German handbook, "Very generally," says Oesterlen, "preparatory measures delay the cure quite unnecessarily, as the quinine is often the best means to remove various disturbances along with the fever; especially, do not be deterred by some vague symptoms, as the state of the tongue, or swelling of the spleen or liver, from using quinine at the earliest stage, if the fever be a bad one." In the West Indies among H. M.'s troops as early as 1832, Dr. Binns always observed the practice of giving quinine in the early stage of fever:* and it is hardly necessary to say, that besides its not infrequent use in this way for many years in the East Indies, including Ceylon and China,† a whole host of French and American writers have, during the last 15 or 16 years, been giving accounts of the exhibition of quinine in immense doses, regardless of the presence of congestions. As an instance of this change of practice, I may quote Celle, *Hygiène Pratique*, 1848. He practises in Mexico, and says, of what we should call jungle fever,—“Where we used to purge and bleed to the great detriment of the patient, a few doses of quinine effect a cure,”—and in 1847, Dr.

* *Lancet*, 1846.

† The reader will find some account of the history of the use of quinine in India in remittent in “*Bengal Dysentery*, 1850.”

Bartlett, the American author of a work on fevers, lays it down, that quinine is to be given at once, without waiting to remove congestions—adding, the quinine removes them; or to quote Oesterlen again—“ In conditions of excitement and congestion, even in inflammation of various parts, so soon as they show an intermittent or even a distinct remittent character, quinine is of great service.” Exactly the doctrine laid down by the experimenter as something new, that if “quinine be given freely, these imaginary inflammations vanish.”

So general, however, do I believe the opinion in India to be, that the complications of fever are not simply inflammatory, and so generally diffused is the knowledge that the quinization treatment,* *i. e.*, by very large doses of quinine, is one effective method of treating tropical fevers, that I should scarcely have thought it necessary to cite the preceding authorities, had not the proposal of the Report, on the treatment of fevers, been considered by some, a more decided step than had ever been previously taken by any systematic writer.

The question of the expediency of adopting that or any other indiscriminate mode of treatment, is not now under consideration; but before leaving the subject, it may be remarked, that the experimenter in his former *brochures*, so far as my memory serves me, appeared to be quite as well satisfied with the effects of 8 or 10 grain doses, as he now is with scruples, and that Dr. Dundas, the latest English writer on the quinine treatment of fevers, is confident of the success of 10 grain doses in intermittent, remittent and continued fever equally; and that in continued fever with the aid of general support, it seems already to have been found efficacious, like the old bark and wine treatment.†

* I prefer the word quinization to cinchonism, or quininism.

† See Dublin Journal, 1852.

(d). *Mixed Treatment*.—The experimenter says—"His fevers were not cured by quinine mixed with ealomel and purgatives, but by quinine alone ;" again, "as for other remedies to assist, I have used none of them, blisters, leeches, purgatives." Now without criticising in detail his fever cases, I have looked over the Hospital records, in which his treatment is registered, and I think that if he had an opportunity of referring to the records kept by himself, he would be inclined to modify the statements quoted above, for he has noted in his own handwriting the use both of venesection and of local bleeding ; *haustus purgans* occurs constantly in his journals ; nor are *Pulv. Jalap. C.* and *Ol. Ricini* absent—and he also used *haustus diaphoreticus* frequently, apparently when he suspected the fever to be eruptive.

It is, however, practically speaking, a matter of no importance, that in fever the experiment was so mixed a one—as it was well known, before the experiment was made, in short as completely ascertained as any fact in medicine, that fever might be successfully treated with large doses of quinine, without bleeding, purgatives, or even aperients being thought necessary.

(e.) *Arsenic*.—This is a very old remedy, which was used by the Chinese and Hindoos in fever. There is a very natural prejudice against the use of so active a poison, but in practice its careful employment has generally been considered quite safe. The experimenter tells us gravely "that all experiments made on the treatment of skin diseases, show it to be innocuous." Very true, and besides its familiar use in cutaneous diseases, besides its having been a popular remedy in fever for the last three quarters of a century at least, in England and in Germany, its use was revived in France about 10 years ago, and since then it has been employed most extensively and successfully in that country, in Italy, Africa and the Brazils. What

does Masselot* say of the treatment of bad remittent?—"In the present state of our knowledge, I should give quinine, if I had to choose between it and arsenic. But if I had both remedies at my command, I should give the two together." But we Indians, who are said in the Report "never to use it," are not entirely ignorant of its virtues, and have employed it, seemingly unconsciously that we were doing anything much out of the way, when the supply of quinine ran short, or when quinine failed to stop the fever.†

The experimenter also informs us that "he has good reason to think that arsenic gives immunity from future attacks of fever." This is just what Boudin, Maillot and other writers say, and mention as one of the particular advantages of its exhibition. Again "now all this, novelty as it is, is no conjecture." It is undoubtedly no conjecture, in fact no more conjecture than novelty, as hundreds of practitioners can testify, as was shown in the Hospitals of Brussels after Waterloo, and as it has been of late years proved on the large scale in France, when Boudin found it succeed in 2,947 cases of fever, 2,000 at least of whom had been previously treated with quinine. I need not enter here on the question of its proving an efficient substitute for quinine.‡

(f.) *Statistics*.—Before leaving the subject of fever, it is agreeable to be able to say, that all the tables of the Report, (that of all the European troops in Bengal for 20 years from

* Archives de Médecine, 1846.

† In 1841 I alluded in the Medical Gazette to its successful and safe employment.

‡ I would venture here to throw out a hint for the experimenter's future investigations. He has tried fever successfully by the quinine and by the arsenic test. He has tried dysentery, he considers successfully, by the quinine test. The arsenic one should, according to *his theory*, be next applied: if it succeeds as in fever, he may be able to banish quinine from India, and possibly even the long tube also.

the large number of facts it represents, is a valuable one,) agree in showing, that though the percentage to strength of cases of fever has not varied much, a distinct diminution of mortality in fever has taken place. The percentage of mortality in the 20 years ending with 1849, taken in periods of 4 years, has been,

General Hospital.	Garrison of Fort William.	All European Troops in Bengal.
9·5	4·5	3·06
8·56	3·1	2·52
10·5	2·2	2·40
5·4	3·1	2·32
6·5	3·4	2·07

In all these columns it is to be observed, that there is a smaller mortality in the last 4, than in the first 4 years of the period, but it is also apparent, that it is only on the large scale of the whole European Army in Bengal, yielding for those 20 years an average of nearly 9,000 cases annually, that the improvement is at all uniform. No one, I believe, will doubt that this gradual change, this diminution of mortality by one-third, is owing to the less free use of depletion, and the freer employment of quinine.

DYSENTERY.

(a.) *Quinine*.—1. The experimenter “has proved the marvellous effects of quinine in hæmorrhagic dysentery.” “This is my own, all my own, for who has ventured on it before?” Unfortunately for these pretensions, the idea that because dysentery is probably a malarious disease, therefore it should be cured by quinine, has long been entertained. But the efficacy of that remedy in this disease is only partially established. Dr. R. Williams writes “So uncertain are the laws of paludal poison, that as a general rule in dysentery, the

exhibition of quinine in any form or quantity has proved rather injurious than otherwise." I am happy to say that this is erroneous, for it has been for many years given with advantage in small doses in chronic dysentery at the General Hospital, and no doubt elsewhere,—and the present Report alludes to its frequent use in hæmorrhagic dysentery in H. M.'s 80th. And as to the statement, that no one has ventured on it before, "*pereant qui ante nos, nostra dixerint*," here again the pertinacious Frenchman, envious of the glory of *perfidè Albion*, has been beforehand with the experimenter. "Sulphate of quinine," says Grisolle, "is indicated, where dysentery shows the character of paludal fever. In such cases I have seen its heroic use as successful as in bad intermittent."

2. Great stress is laid on this successful treatment of dysentery by quinine. The Report says, "It is directly a practical matter of fact, and cannot be answered." "I must have had increased mortality if *my* theory is wrong, whereas I have had far less." Now, waiving the question whether his treatment must have had the effect he supposes, I find myself, to my great surprise, in a position that enables me to tell him, from his own books, that he has misapprehended the matter, that it is *not* a matter of fact, and that there *was* increased mortality in the General Hospital in his own hands, under that mode of treatment. The experimenter explains that by hæmorrhagic dysentery, he means "all forms of it in which much blood is voided by stool—not only the worst forms of it:" his statement therefore may be considered to be of pretty general application to Bengal dysentery.

3. The statement that seruple doses cure hæmorrhagic dysentery being a very important one, and such experiments as I had myself seen made with it being failures, I examined the books of the Experimental Ward, to see how its success-

ful results had been obtained, and the following results of the examination of them, are as near an approach to the truth, as I am able to make.

There were treated without quinine about 30 cases of dysentery; of these 3 died, 1 with hepatitis and diarrhœa, and 1 with hæmorrhage; 27 were discharged, of these 5 were re-admitted. There were about 32 cases of dysentery treated freely with quinine; of these 6 died in the Experimental Ward, one of them of hepatitis; and there were transferred to the other wards 5 cases, (2 of them fatal.) There were discharged, 13 absolutely cured, 4 convalescent, 4 well, but who had passed blood within two or three days of their discharge;* or to view the matter generally, among 30 cases treated without quinine, there were 3 deaths; among 32 cases treated with it, there were 6 deaths, or the mortality was actually increased during the quinine treatment,—I do not say by it.

The reader ought also to know, that at the same time there were some 23 cases of diarrhœa treated freely with quinine, of whom 21 were cured, 1 died of cholera, and 1 was transferred: about 12 cases of diarrhœa appear to have been treated without quinine, and with no deaths.

Or to take a still more general view of the whole case, 55 cases of bowel complaint treated with quinine were attended with 8 deaths, (excluding one of cholera,) and 42 cases treated without it, with 3 deaths.

* Patients may be discharged in this state from any of the wards. If a sailor is convalescent, and his Captain wishes to have him, on leaving port, it is often necessary to discharge him. The state in which many of the dysentery cases were discharged, according to the books, has some bearing on the vaunted quick cures of the Experimental Ward: but it is unnecessary to open up a question, on which, from the nature of the experiment, it has been seen that there can be no distinct facts.

(b.) *Mixed treatment.*—As in his remarks on his treatment of fever, so in those on the management of dysentery, the experimenter seems to have expressed himself carelessly: he says, “I have used no other remedies but washing out the bowels with water, and giving 6 or 7 scruples of quinine daily”: and this is the impression which the whole Report conveys, for though in one place he says, “for the acute form of dysentery bleeding is decidedly necessary,” and “a full dose of laudanum will be found of the greatest benefit,” he nowhere tells us, how to distinguish the paludal from the acute form, and this statement is soon forgotten, among such general expressions as these, “leeches, blisters, and ipecacuanha I know nothing of;” “neglecting all these, bleeding, &c.” The tabular returns also make no distinctions of bowel complaints, according to their treatment.

From the comparison that has been made in the preceding section, the inaccuracy of these statements might be inferred, and the books also shew, that besides using venesection more freely than it was practised in the other wards, the experimenter employed occasional leeching, and very frequently opium, morphia, nitrate of silver, acetate of lead, all in large doses, and chalk mixture. Are these remedies to be looked on as part of the treatment or not?—for, although in an early part of the Report, he says “he may show the effects of alum and such like harmless palliatives,” we do not afterwards hear anything more on the subject. If any positive conclusion can be drawn from so small a number of facts, and I am very far from supposing that it would be safe to do so, it follows from those adduced in the last section, that the quinine treatment was less successful than the other, both in dysentery and in bowel complaints generally. It is therefore to be regretted, that the experimenter did not analyze more carefully his own cases, before making such sweeping state-

ments, as to the efficacy of quinine, and the non-employment of other remedies.

(c.) *The Long Tube*.—The earlier part of the Report and the experimenter's previous *brochures*, are full of the long tube and of its virtues. In former times, and before he had discovered the use of quinine in dysentery, the long tube and doses of laudanum were his panaceas, and were talked of, perhaps more confidently than his more recent mode of treatment. He paid his professional brethren the compliment of saying that "he would cure hundreds of the slighter cases, that perish under the present treatment." In those days he expressed his belief in the common existence of *scybala*; he then seriously expressed his opinion, that, as the conjunctiva and the lining of the intestines were both mucous membranes, the application of fecal matter to the inner surface of the bowels must be as irritating to it, as it would be to the conjunctiva of the eye. He also compared his large injections to the treatment of open abscesses, and said that his treatment was in fact turning the internal abscess of the colon into an external one.* But although he says "he need not repeat his already published opinions of the theory of acute and chronic dysentery," more extended experience seems to have made him practically drop those false analogies, as we hear nothing more of them. He now limits himself to the remark, "that you must wash out all the irritating matters which the large bowel contains, and which are the cause

* Mr. Hare, on Dysentery, pp. 8, 9, 10, 11, 21, 30, 23; in Bengal Dysentery, p. 50,—it has been stated that "*scybala* or accumulations of feces appear to have been scarcely ever observed during life, and never after death." This as it stands, is a general expression of the truth, and a stray instance or two to the contrary serve merely to confirm the fact. The statement would however have been more exact, if the expression used had been, as was intended, "accumulations of (hardened) feces." This explanation was necessary, as many people attach a very vague meaning to the term *scybala*.

of its sloughing." Passing by this opinion as to the cause of sloughing,* which we leave it to him to reconcile with the paludal theory of dysentery, we observe, that towards the close of the Report, he speaks less urgently of the long tube, and says "it need not be passed up oftener than once in eight or nine cases," "although he always prefers using it himself," and says "the necessity of injections has been still more impressed on him." We also hear less of the spasms or narrowing of the sigmoid flexure, on the existence of which the use of the introduction of the long tube mainly depends.

In fact, notwithstanding the Board's condemnation of those who do not carry out the experimenter's mode of administering enemata, the Report to my mind gives the impression that quinine is much more efficacious than the long tube. It is a pity that the Report has not been more explicit on this head, that the use of that instrument has not apparently been studied, apart from other remedies. No one can collect from the Report, how often

* Notwithstanding the opinion expressed by an intelligent reviewer in a late number of the British and Foreign, it still appears to me, that the ordinary process in acute Bengal dysentery is more one of infiltration, death and sloughing of the mucous coats, than one of regular progressive ulceration of the solitary glands, as described by Parkes and Murray. There is scarcely time for the latter process. The case quoted by the reviewer from "Bengal Dysentery," as probably giving a fair average representation of the usual appearances in Bengal dysentery, was quite an exceptional one, although I have seen similar ones.

The descriptions of the alterations of the bowels in dysentery given by Rokitsansky, the Irish writers in the Dublin Journal, and by Dr. Baly, agree in all their main features with those of Bengal dysentery, the degree of liver affection alone excepted. Rokitsansky's ulcerative inflammation of the follicles brought on by diarrhœa, appears to correspond with the dysentery of Parkes. It is a curious fact that in the earlier months of this year more than half the fatal cases of dysentery in H. M.'s 80th were attended with abscess of the liver, while in the neighbouring Hospital abscess was not common. I have never heard of liver abscess in children connected with dysentery.

it was passed up beyond the sigmoid flexure, how much was actually attributable to the use of the long tube, and how much to the other remedies which were applied.

In short, the Report appears to me calculated rather to advance the cause of quinine than that of the long tube, and it is curious to observe, how the present state of things in India agrees with that in Europe, as represented in the Library of Medicine, 12 years ago—"Dr. O'Beirne is anxious to induce his professional brethren to make use of the introduction of his favorite tube into the sigmoid flexure, believing that much mischief results from the detention of fecal matter and of diseased secretions in the large intestine, mainly caused by a spasmodic closure of the upper annules of the sigmoid." Dr. O'Beirne, however, as far as I can judge from books, has, like the experimenter, scarcely remained true to his first convictions, for he now urgently recommends tobacco, but in the form of fomentations, therein at once agreeing with, and differing from Matthews, who also administered tobacco, but by means of a hookah snake, which seems to have been the great prototype of the long tube. For myself, after giving large injections a fair trial, I cannot assign to them a higher place in Bengal dysentery than that of frequently useful adjuvants: and however useful they may be in certain conditions of the bowels, as in constipation, or in the incipient stage of dysentery in the Upper Provinces, the general result of what I have heard from experienced practitioners, who have given them a fair trial in European Hospitals—(I believe I may mention particularly that of H.M.'s 80th, whose Surgeon is so favorably mentioned by the Board) agrees with the opinion of them that has been just expressed.

"Quin si morbus diutius excurrat, frustra erit (me judice) qui medicari sataget, vel quavis methodo prædicta, vel elys-

teribus, abstergentibus, agglutinantibus, atque astringentibus, quæ pro variis ulceris injici solent.”—*Sydenham Obs. Med.*, iv. 3, 19. And this, I fear, is every practical man’s view of the case to this day.

(d.) *Calomel Treatment of Dysentery*.—1. As the mercurial treatment is not a recommendation of the report, it is unnecessary to allude at much length to this subject; still I shall examine one or two statements of the Report on this head. The experimenter, who in a former *brochure* spoke “of the dreadful results produced by the system of salivation, which is still the common routine in India,” now expresses himself thus: “The mortality is proved by these tables to have increased by regular progression, and the case books also show that the number of cases salivated has also decreased in the same proportion, till now, when salivation is never resorted to, the mortality is actually doubled.” “How greatly is the demonstration strengthened, when the same fact occurs in the Regimental Hospital with a class of patients entirely distinct in every circumstance and habit from the former.” It may possibly be thought a sufficient reply to this, to quote the words of the experimenter at the close of the Report. Alluding to modern practice he says, “The mortality has increased to what it was when scruple doses of calomel were in use.” But as the experimenter has just stated, that since salivation was given up, the the mortality has been doubled, and now says that under either system it is the same, what are we to regard as his real opinion on the subject?

2. However, leaving him to reconcile those warring opinions, I proceed to the facts of the case, and have compiled from the experimenter’s own tables, the percentage of mortality from bowel complaints for the last 20 years, in periods of 4 years, for here as elsewhere diarrhœa and dysen-

tery classed together, will probably give the most trustworthy general results.*

Year 1830-35. 1834-37. 1838-41. 1842-45. 1846-49.

Genl. Hospital,	10·6	19·6	17·8	17·7	18·1
Garrison,	9·6	6·1	5·3	6·1	6·6

Can any one trace a regular progression in these figures? So far from this, there appears to have been no change of any importance for the last 16 years. Again, do they show a progressive common deterioration? So far from this, we find, that the Regimental Hospital has been much more successful in the last 4 than in the first 4 years of the period, whereas it is exactly the reverse with the General Hospital.† But even if the returns had corresponded, the very fact of their doing so, while the Regiments in Garrison were constantly changing, each new Surgeon bringing his own particular practice with him, would have been sufficient to my mind to

* As some may wish to see how the figures stand for dysentery alone, I give them also:

1830-33. 1834-37. 1838-41. 1842-45. 1846-49.

Genl. Hospl.,	15·3	25	21·2	25·6	27·6	$\left\{ \begin{array}{l} \text{or accord-} \\ \text{ing to my} \\ \text{returns,} \end{array} \right\}$	26·4
Garrison, ...	11·1	6	6	12	16·1		

In these figures can any one trace a gradual regular progression, or a correspondence between the two series, such as can be attributed to a progressive change of practice? So far from the mortality being regularly progressive, it was 14·8 in 1833 and 26 in 1834 in the General Hospital.

† Of these anomalous facts as they stand, I can offer no satisfactory explanation: probably, if any accurate data for the years immediately preceding 30, could be got, it might be possible to explain them. How Raleigh, writing in 1842, called the mortality 10 to 14 per cent. I don't know; in the eight previous years it was 22 per cent. Martin's tables make it 16·2. Raleigh says, that at the end of the last century, when calomel with large doses of opium and mercurial frictions was the most common practice, it was from 30 to 50, nay 75 or 80 per cent. It is much to be regretted that the labours of the white ants make it impossible to get any accurate account of the mortality in former years.

shew, that their accordance depended on some more general influences, than those of treatment.

3. Ere leaving this subject, it is curious to look back at the last experiment at the General Hospital, that of 1816. In those days Dr. Halliday, shocked at the abuse of calomel, of which he says 13,237 grains were given in the General Hospital in one month, thought that he had discovered a more successful mode of treating dysentery than by calomel. He thus compares his own, or the non-calomel treatment, with that of his colleague, Mr. Wood, or the calomel practice :

Dr. Halliday	had	76	cases	dysentery,	4	deaths,	mortality	5·2	per cent.
Mr. Wood	„	24	„	„	9	„	„	37	per cent.

We find therefore that the non-mercurial treatment of 5·2 per cent. was more successful than that of the late Experimental Ward of the General Hospital, which was, according to the official report, 7·3, but that the mercurial treatment of Mr. Wood, with a mortality of 37 per cent., was scarcely so unfortunate as that of the Surgeon of H. M.'s 70th Regiment, who lost 42 per cent. These comparisons may appear somewhat ludicrous, but they are not in reality juxtapositions of more dissimilar things, or of more inaccurate data, than those which are classed together in the general tabular statement of the Report, and they afford additional illustrations of the extraordinary results that may be obtained by the comparison of figured statements, when not carefully conducted.

(e.) *General Statistics of Dysentery.*—But though, for reasons above stated, I cannot assent to the hastily assumed generalizations of the experimenter, it is a fact that there has been no progressive diminution of mortality in bowel complaints. On the contrary, it will be seen from the returns of the whole European army in Bengal, which for the last 20 years has furnished an annual average of 4,011 cases

of diarrhœa and dysentery, that the percentage is somewhat greater than it was 16 years ago, greater by about 1-6th.

1830-33.	1834-37.	1838-41.	1842-45.	1846-49.
5·04	4·99	6·63	6·3	6·2

It is extremely unsafe to generalise in statistics, but I cannot help remarking that 1838-41, the first period of increase, and the period of greatest mortality, comprehends the Affghan and China campaigns. It could also scarcely be expected, that there would be a diminution of mortality, as the troops have been much more engaged in campaigns, and in occupying new stations, during the last 12 years, than during the first 8 of the period : besides, there has been no important improvement made in the treatment of dysentery, at all events extended enough to affect general returns, as there has undoubtedly been in fever.

It is also well worthy of observation, that from the general returns for the whole European army, if we compare the first 4 years of the period of 20 years, with the 4 last of it, it appears, that the proportion to strength of cases of bowel complaint has considerably increased, that of diarrhœa having increased greatly, and that of dysentery having rather diminished, while, as we have already seen, the proportion of fevers to strength has remained nearly stationary.

QUININE—ITS SAFETY?

THE experimenter states, that "he has never seen any harm result" from reducing a patient to a state of quinzation, and the Board re-echo "its perfect safety." This is the frequent testimony of writers who have used the drug freely. Bally, for instance, mentions giving 15-grain doses 4 times in the 24 hours, without the slightest inconvenience being produced, on which Trousseau and Pidoux remark, that either Bally observed his patients carelessly, or that his patients deceived him. But are there no exceptions to this rule? Are there not, besides many cases of more or less permanent deafness and blindness, several instances of still more disagreeable effects on record?* and have the French experiments in the treatment of rheumatism with it been forgotten? In Florida, in particular, where the system of monster doses has for some years been the routine in fever, disastrous results have not infrequently followed its indiscriminate exhibition, especially in children. The most commonly recorded effects on man, are amaurosis, deafness, delirium, muscular tremor and coma, and recent experiments on animals mention as some of the symptoms that precede death, great restlessness, speedily followed by muscular agitation or tremulous movements. To small animals it readily proves

* Piorry is said to have lost 6 cases of intermittent fever, and the other Paris Hospitals at least 12 more from its injudicious employment, and the same has happened to Giaconini and others in Italy. *Gazette des Hôpitaux*, No. 123, 1847. Alibert and Reccamier have seen deaths caused by 15 grain doses. Oesterlen remarks, that he has always used quinine in moderate doses with perfect safety, but that its employment in a Parisian Hospital seems to be quite another, and a dangerous thing.

fatal, but only rarely to man. The experimenter mentions the frequent production of amaurosis and deafness, were they never the precludes to the further effects of the drug?

He says the man who died of fever in H. M.'s 70th "had been drinking, and had strong symptoms of delirium tremens." Does this mean that the man died of fever or of delirium tremens? and if of the latter, has he never observed quinine induce a state of tremor, a *delirium é quind*? strongly resembling *delirium é potu*, or at least has he not seen it aggravate an already existing tendency to delirium tremens?

One remarkable case was transferred from the Experimental to the other wards, as a case of delirium tremens, who presented the symptoms of complete quinization, with deafness, tremor and coma. The patient died. In this case, after death the coats of the stomach were found pale, not even reddened, or with any abrasions of the mucous membrane, such as are almost always found in *delirium é potu*, which occurs usually in old drunkards with more or less diseased stomachs. This was a rather startling case, but there were some other striking incidents that deserve a short notice.

It is somewhat remarkable that in a ward devoted to fever and bowel complaints, two cases, both fatal, of ebrietas should have occurred, and also two fatal cases of epilepsy and of apoplexy, (of course there is nothing surprising in the case of apoplexy, if it was merely the termination of a fever, but, although the case was admitted as fever, it does not appear as such in the returns.) I also think it surprising, that of six cases of delirium tremens, which occurred in that ward, and which were treated more or less freely with quinine,* (for the two cases of ebrietas, I suppose must be counted as deli-

* All of these patients had taken 1 or more scruple doses.

rium tremens,) no fewer than five terminated fatally in one or other of the wards.

The suspicion that quinine might have acted injuriously, has evidently crossed the mind of the experimenter, for in the case of apoplexy, just alluded to, he expressly explains, that the patient had stopped taking quinine, "as he carelessly thought him convalescent." But when does he seem to have stopped taking it? "This very stout man, who does not seem very bad," appears to have been taking from the 10th to the 12th, 4 scruples of quinine daily, and this was only stopped the evening before his sudden death on the 13th. This at least so far as the Hospital records show. The experimenter candidly admits the bad effects of large doses of quinine in eruptive fevers, the good effects of which in smaller doses many authors record; possibly on reflection he may admit, that it may also be injurious, where there is a tendency to delirium tremens, or other cerebral disturbance.

We have already seen that the total mortality among all cases that were treated at all in the Experimental Ward, exceeded that of the other wards, and until the cause of that mortality is satisfactorily explained, the effect of the incautious use of quinine offers itself as a possible explanation of it: and the possibility of this being the true solution, is strengthened by the fact, that the bowel cases admitted into the Experimental Ward, and treated with quinine, yielded a mortality of 14.5 per cent., while those treated without it gave only one of 7.1 per cent. (as almost all fevers were treated with quinine, it is only possible to make this comparison in bowel complaints.)

We have also just seen reason to suspect, that quinine proved deleterious in delirium tremens, and in tendency to cerebral congestion, as well as in eruptive fevers.

All this is matter for grave consideration, but I would

avoid hasty generalization from a small number of facts, and should hardly have felt called on to say so much on the subject, had not the Medical Board, notwithstanding that they had previously given a caution on the subject, "deploring the *possibility* of the occurrence of serious consequences," which in all probability had *actually* occurred under their eyes, taken this opportunity of assuring the profession, of the "perfect safety of exhibiting quinine in large or scruple doses repeated three, four, six times or oftener in the 24 hours, in fevers of all types, the product of the poison of malaria."

ἐμοὶ μὲν οὐ πιστὰ λέγοντες, ἄλλω δέ τι.—HERODOT.

It is impossible, even with the limitation, which they have added, that such indiscriminate practice should not continue, as it has done heretofore, occasionally to produce disastrous effects, as it constantly will, disagreeable ones. They seem to think nothing of driving a patient distracted, or half mad (as he usually expresses himself) by singing in the ears, irritability of the stomach, deafness and partial blindness,* and this while there is every reason to believe, that smaller doses will produce every effect of the drug that we wish to obtain.

Though not agreeing with them in their opinion of its perfect safety, I would, however, go so far with them, and say, that it is certainly a very difficult thing to give quinine to an adult during a single attack of fever in permanently injurious doses, for large doses, such as they mention, make the head so uncomfortable that the patient will no longer take them, or if he does take them, they produce such irritability of stomach that each dose is usually rejected.

* In some cases in the General Hospital, patients have been treated expressly for the effects of quinzination.

CONCLUSION.

BUT these remarks must draw to a close. After all that has been said, I trust that every one who has perused these pages, must feel satisfied, that the experiment was not conducted with sufficient care; that the tables are inaccurate, and do not bear out the interpretation of triumphant success, which has been put on them : that the treatment was a mixed one, not admitting of any accurate conclusions being drawn regarding the effects of individual remedies, or if, of any, of conclusions regarding bowel complaint, directly the reverse of those of the experimenter, and finally, that there is no originality in the views put forth in the Report.

In short a very distinct reply has been given to a question which was propounded by the Medical Board, in the year 1816, when Dr. Halliday's experiment was under consideration—"Was there any novel mode of practice introduced, was the nature of that practice beneficial, and such as to render its general adoption desirable" ?*

What then, it may be asked, has the experimenter effected nothing?—have his enthusiasm and industry been thrown away? By no means: the agitation of the question of the employment of quinine has given a considerable impulse to its use in Bengal, and in so far it has done good. It is far better to give quinine too early than too late, too

* The reply given on that occasion was, that "Dr. Halliday's patients were discharged, often not cured, or even convalescent." In the present instance, there can be no distinct facts on this head, regarding any of the Wards of the General Hospital. The Official returns we have seen, afford a strong presumption, that the cases in the Experimental Ward of H. M.'s 70th must have been slight, because the portion of the regiment, treated by the experimenter, produced a larger number of cases, than the main body of it.

much of it rather than too little. It is a remedy which almost any one can give with safety, for, although I have seen with disapproval mothers giving their children 4-grain doses of it in dentition, and I have known patients, in dread of fever, often make themselves very uncomfortable with it, and seen a whole series of symptoms of deranged digestion and nervous disturbance follow its continued abuse, there is little risk of its being generally given in the monster doses of the Experimental Ward.

I would fain hope also, that there may be some foundation for the belief in the high efficacy of quinine in hæmorrhagic dysentery, for as yet I know of no medicine, that really makes an impression on its bad forms, and indeed a knowledge of the structural changes that take place, scarcely encourages the hope, that one will be discovered.

Next, as to the long tube, though that instrument is not likely to be often used, attention may have been awakened among those who were not familiar with their employment, to the use of enemata, remedies by no means to be neglected in the treatment of dysentery.

The experimenter also deserves credit for the industry with which he scrutinized the records of the General Hospital, and he has shown considerable ingenuity in the way in which he has endeavoured to support his theoretical opinions by numerical statements.

Before closing these observations, I must apologise to the reader for having in the last part of them, perhaps brought the experimenter too prominently forward, and so frequently examined his crude speculations, especially as I know that in this country *his system* has very generally been silently laughed at. My apology for this must be, that the Report consists mainly of his opinions, and that it was hardly possible to deal with the one apart from the others.
